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Documento de Trabajo Nº 289

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new evidence on Efficiency and Peer Effects.

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Abstract

We estimate the treatment effect associated with attending a private instead of a public voucher school in the Chilean voucher system. We find a large and significant positive treatment effect. We analyze the influence of peer effects on our result by estimating new treatment parameters that control for peer group characteristics. When we do so, we still find a positive treatment parameter that is large in magnitude and statistically significant. Hence we conclude that the positive treatment effects are not due to peer effects and sorting, but rather stem from the greater efficiency of private voucher schools.

I. INTRODUCTION

Studying the Chilean education voucher system is of great interest in assessing the theoretical literature on the advantages and disadvantages of the voucher system. In this paper we estimate the treatment effect associated with attending a private voucher school instead of a public one, using the 2002 test scores of 4th grade students in elementary school.

The voucher system was introduced in Chile in 1982. It includes both public schools that are run by the municipalities, and private subsidized schools. In addition to the voucher, some municipal schools (MUN for short) receive extra funds both from the Ministry and/or the municipality[1]. Private subsidized schools (PS for short) can also receive extra funds from the Ministry, and/or charge fees from parents (up to a limit).

To evaluate the effectiveness of private versus public voucher schools, it is important to compare schools with similar budgets. For this reason, we focus on schools that charge low fees (or no fees at all) when we estimate treatment parameters and hence are financed almost entirely by the voucher. Working with these schools we find a large and significant positive treatment parameter. Since some authors claim that sorting and peer effects are important factors in how voucher systems work, this positive and large treatment effect could be the result of these effects, and not the consequence of greater private voucher school effectiveness.

[1] These funds are either channeled through programs handled by the central government, or consist of subsidies bestowed by local government to cover deficits.

To analyze whether our results are dependent on peer effects, we re-estimate the treatment parameters controlling for peer group characteristics. If the positive treatment effect estimated without controlling for peer group characteristics were exclusively the result of the sorting process and peer effects, this new treatment parameter should be zero. However, when we condition on peer group characteristics, we still find a gain associated with attending a private voucher school instead of a public one. That is, the effect of treatment on the treated or *TT* is still large in magnitude and statistically significant, illustrating the robustness of our results.

II. LITERATURE REVIEW

Much of the early empirical literature evaluating the voucher system in Chile suffered from methodological and/or data limitations[2]. This literature used the results of a standardized test taken every year since 1987 in all schools as a measure of output. The test is taken by different grades: 4th and 8th grade in elementary school, and 2nd grade in high school, in alternate years. Up until 1998 no individual data was available for this test (called SIMCE). Hence empirical papers used the school as the unit of study (see for example Mizala and Romaguera (2000)). Additionally, these studies lacked good information on the socioeconomic characteristics of the students, and had to use proxies to approximate average characteristics of the school. These limitations meant they were unable to correct for selection bias in the estimation of the treatment

[2] For a discussion of the empirical literature in Chile, see Sapelli and Vial (2002).

effects[3]. More recent literature (for example Contreras (2001) and Tokman (2002)) uses individual data and corrects for selection bias. These studies found significant differences in scores between public and private voucher schools. However, they did not take into account that some public schools (i.e., municipal schools) receive additional resources from the government, through municipal transfers or through participation in special government programs.

Sapelli and Vial (2002) take these differences in school budgets into account. They separate geographical areas according to the amount of per capita additional funds that public schools receive from the government (in addition to the voucher). In areas where public and private voucher schools receive similar per capita subsidies (where public schools receive up to 25% more funds than private voucher schools), they find a positive and large *TT*[4].

Mc Ewan and Carnoy (1998) and Hsieh and Urquiola (2002) argue that the main result of the introduction of vouchers in Chile has been sorting. Hsieh and Urquiola show that higher enrollment in private schools coexists with lower test scores in public schools in the same municipality. The key problem with this study is that it ignores the issue of causality: the negative correlation between test scores and private enrollment could be either proof of the peer effect, or alternatively proof that entry is endogenous and occurs first where municipal schools are doing a poor job (see Hoxby (2001)). Gallego (2002)

[3] Another common methodological problem in this empirical literature is the inclusion of school inputs in the estimation, confounding the estimation of production functions with the estimation of treatment effects.
[4] They used 1998 SIMCE results, for second grade in high school.

finds that the issue is crucial: results with and without controlling for endogenous entry differ significantly. After controlling for endogeneity, he finds that competition from private subsidized schools *increases* the test scores of municipal schools[5].

In any case these papers pose the question of whether positive TT's are the result of sorting and peer effects. We intend to answer this question.

III. ESTIMATING TREATMENT EFFECTS

In this section we estimate treatment parameters using the normal model[6]. We use **individual** data for the test taken in 2002 by the fourth grade of primary school[7]. A separate, simultaneous survey, that can be matched to test results, provides the data on individual socioeconomic characteristics. We also have information on the characteristics of the schools and the amount of money that each municipality gives to the schools they run. Finally, we use aggregate information by geographical area from the CASEN national survey taken in 2000.

The main advantage of using 4th grade (rather than older students') test scores is that at this level, parents usually choose a school in the same area they live in. This fact is central to our identification strategy, as will be seen when the validity of our exclusion restriction is discussed. It is also important that

[5] Note that Gallego obtains the same results as Hsieh and Urquiola when he does not control for endogenous entry.

[6] For an explanation of the normal model, see Heckman, Tobias and Vytlacil (2000).

[7] We exclude schools in rural areas, and students who attend special schools (for example, schools for blind or deaf students).

since 2000, parents declare how much money they pay to the school[8]. This allows us to focus our attention on schools that charge low fees, and hence are more similar to "pure" voucher schools, working with similar budgets.

The Normal Model

We assume that, given school characteristics, potential test scores in public and private voucher schools (Y_0 and Y_1 respectively) are determined by the following individual-level characteristics: family income group, education of the student's mother and father, family size, age and gender of the student, whether the student has failed a grade and whether he/she received pre-school education. Public schools and private subsidized schools exploit these characteristics differently, so potential test scores in each type of school may be different for a given student. We assume that potential test scores can be represented by the following output equation:

$$Y_D = X\beta_D + u_D$$

Where D is defined as D=1 if the student chooses a private voucher school, and D=0 if the student chooses a public school.

The selection rule is defined as:

$$D=1 \text{ if } D^*=Z\theta+u>0; \quad D=0 \text{ otherwise.}$$

D* denotes the net gain associated with attending a private voucher school. This is the selection equation.

[8] They are not asked to declare the exact amount they pay, but to classify themselves according to a predefined schedule.

The vector of observable characteristics affecting school choice, Z, includes the variables of the outcome equation, X, in addition to a new variable (the exclusion restriction) defined as follows: the ratio of the average school fee charged by PS schools in the year 2000, to the average 1999 language SIMCE test result in PS schools. Note that both averages are ***for the geographical area in which the student goes to school*** (the same ratio is constructed for municipal schools)[9,10]. These ratios can be considered proxies for the average unit price of private subsidized and public education by geographical area (the price per unit of test score). Note that these are not the prices that individual students face, but the average price in the area they where they go to school.

In Appendix A there is a description of the variables used in the estimation.

We assume that the error terms are normally distributed,

$$\begin{bmatrix} u_0 \\ u_1 \\ u \end{bmatrix} \sim \mathrm{N}\left(0, \begin{bmatrix} \sigma_{00} & \sigma_{10} & \sigma_{u0} \\ \sigma_{10} & \sigma_{11} & \sigma_{u1} \\ \sigma_{u0} & \sigma_{u1} & 1 \end{bmatrix} \right).$$

Under this distributional assumption, the average treatment effect (ATE) and the effect of treatment on the treated (TT) conditional on X are defined as follows:

$$ATE(X) = X(\beta_1 - \beta_0)$$

---

[9] We obtain the average school fee from the CASEN 2000 survey. We used 1999 year test scores, because this was the last year that SIMCE was taken by 4th grade students.

[10] We correct standard errors for the grouping procedure. Hence, we assume that unobservables are independent across groups (geographical areas) but not necessarily independent within groups.

$$TT(X, Z, D = 1) = X(\beta_1 - \beta_0) + (\rho_1\sigma_1 - \rho_0\sigma_0)\lambda(-Z\theta)$$

Unconditional parameters are estimated as the average of the conditional parameters over the relevant sample. Standard errors are computed using parametric bootstrapping[11].

Exclusion restrictions

The crucial assumption for the validity of our exclusion restriction is that the price of private subsidized and municipal education faced by the student affect school selection, but not potential test scores. The first part of the assumption is validated by the data (especially for the price of PS schools, where the variance is higher): the probability of attending a private subsidized school is higher in geographical areas where private subsidized education is, on average, cheaper. That is, as with any other good, the demand for private subsidized education rises when its price is reduced.

As usual, no credible empirical test exists as to the validity of the second part of the assumption, that is, that the average unit price of education is not related to the unobservable characteristics that affect test scores. There is no reason to think that the average unit price of private subsidized and municipal education in the geographical area where the student goes to school should be *directly* related to the unobservable student characteristics that affect test scores. But under certain conditions those average prices may be *indirectly* related to the unobservable characteristics that affect test results.

[11] See Heckman, Tobias and Vytlacil (2000)

There are two situations where the latter relationship could emerge: the first is when students can easily go to a school outside the geographical area they live in ("student mobility"); the second is when families can easily choose their area of residence ("residential mobility"). If either student mobility or residential mobility were high, we might expect that families with certain characteristics that may affect test scores (e.g. more informed or educated families), would choose geographical areas where the average per unit price of subsidized education is lower. Hence, if this were the case, the average per unit price of education would not be a valid exclusion restriction. However, as we explain below, we think that this is not the case in our data set.

A special feature of 2002 data is that it is the only available survey where parents are asked their area of residence. Therefore, we know not only the geographical area (municipality) where the school is located, but also the area where the children live. It allows us to test how often children attend a school outside their residential area (student mobility). We find that in fourth grade of primary school, student mobility is very low: 89% of students go to school in their area of residence[12].

We also assert that residential mobility is low, as a result of the large fraction of families in our data set that live in social-program housing, and the legal restrictions they face that prevent mobility. According to the CASEN 2000 survey, around 40% of the students of subsidized elementary schools live

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[12] It appears that mobility is higher in 8th grade, since year 2000 data (which is the last survey for 8th grade) shows that 33% of students report that they had changed schools after 4th grade. There is also some evidence (from a drug survey) that mobility is high in secondary education.

in social-program houses. Since we consider students who attend low-fee schools (usually poorer students), this percentage should be a lower bound of the fraction of students in our data set whose families live in public housing.

Housing policies preclude residential mobility, as shown by Soto and Torche (2002). The authors focus on the fact that the lack of convergence of regional income in the Chilean economy is largely associated with low levels of interregional migration[13]. They argue that this is the result of two policies that affected interregional migration in a systematic way: public housing and regional development programs. Since 1980 housing policies incorporated important limitations to beneficiaries to avoid leakages of subsidies to non-target groups (i.e., high income quintiles). The most important limitations were a prohibition to sell or rent subsidized houses (until 2001) and the existence of rigid norms determining the location of subsidized housing. The authors provide econometric evidence that the prohibition to sell or rent subsidized houses effectively tied families to their original location, and thus strongly reduced residential mobility[14].

Controlling for per-student budget

To evaluate the relative performance of municipal and private subsidized schools, it is important to compare schools with similar budgets.

[13] Soto and Torche show that interregional migration in Chile is very low by international standards, in particular when one considers that the country is small, the population very homogeneous, and urbanization levels quite high. On average, in the 1965-2000 period only 1.3% of the population moved between regions every year.

[14] A second area in which policies play a role is via regional subsidies given to specific economic activities.

There are two sources of differences in the budgets of private subsidized and municipal schools: one is that some municipal schools receive financial assistance from the government above and beyond the value of the voucher, especially through additional funds transferred by the municipalities to the schools they run; the second is that private subsidized schools are allowed to charge fees (up to a limit) through the *"financiamiento compartido"* or "shared funding" regime.

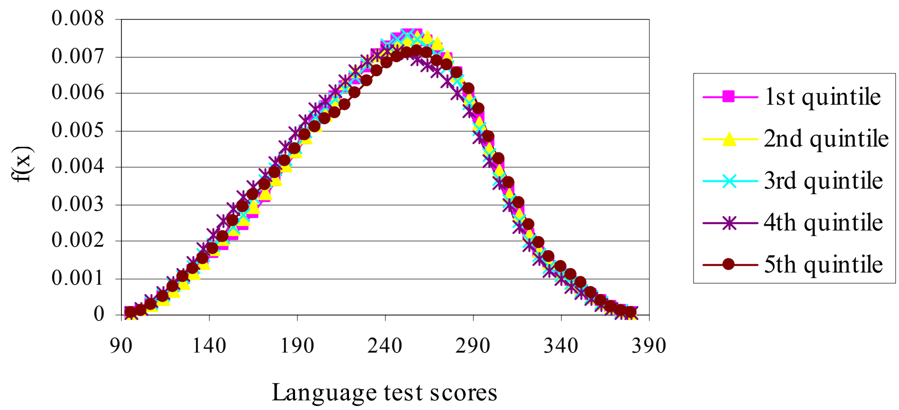
When analyzing the 2002 data (for 4th grade students in primary school), we find that the distribution of test scores in municipal schools is very similar across different transfer groups[15], as Figure 1 shows[16]. Groups shown in Figure 1 were constructed as quintiles: we sort areas according to the amount of additional funds transferred by municipalities to the public schools they run, and form five equal sized groups, each representing 20% of the population of voucher students. Since we find that the distribution of test scores is similar across transfer quintiles, we estimate treatment parameters considering schools in all geographical areas.

---

[15] Sapelli and Vial (2002) grouped geographical areas according the amount of per capita subsidy received by municipal schools, and ran separate regressions for each group. They found that for 2nd grade of high school this separation is important. They found a positive and significant treatment parameter for areas where additional transfers to public schools are low (i.e. in those areas voucher students get higher test scores when they attend a private voucher school instead of a public one) and hence PS and public schools work with similar budgets. They also found a substantially negative treatment parameter in those areas where additional transfers to public schools are high. That is, the public schools that receive the largest transfers perform substantially better than PS schools. This result shows the importance of taking into account differences on the supply side (i.e. in school budgets) in the estimation of treatment parameters (at least for high school).

[16] This may be the result of the way the municipalities assign funds (if they give additional funds to high schools instead of primary schools), or it may be evidence that "money does not matter" at the primary school level. Since we only have aggregate data on funds transferred to public schools, we cannot say which is the proper explanation for this finding.

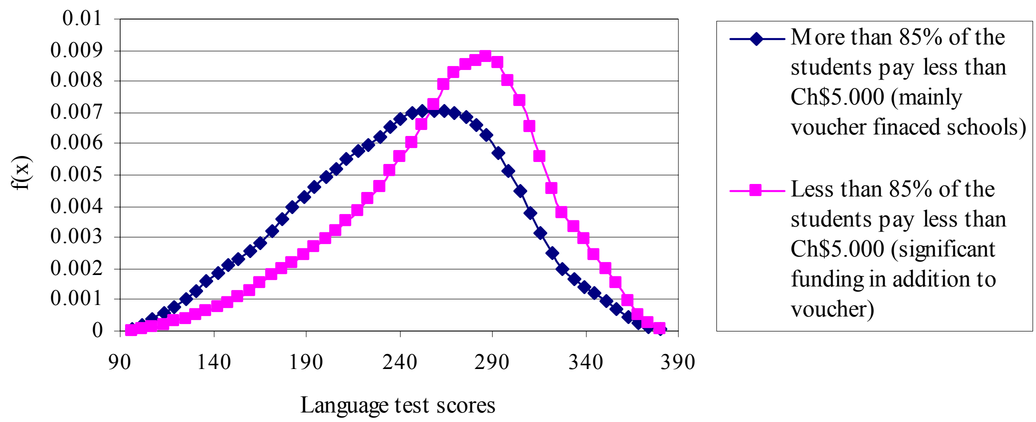
Figure 1: Distribution of language test scores in MUN schools by transfer quintile



When we consider private subsidized schools, we find that the schools that charge higher fees do obtain better test scores. Figure 2 shows the distribution of language test scores when we separate PS schools according to the fees they charge. In the first group we considered all the schools where over 85% of students pay less than Ch$5.000 monthly. This group represents 27% of the population of private subsidized students. They receive on average approximately 5% of their total educational budget from parents[17]. Hence, in the estimation we consider only PS schools that belong to this group, that is, PS schools that charge very low fees (or charge no fees at all). These are the schools that are most representative of a "pure" voucher system, where all schools work with similar budgets, a budget consisting mainly of the value of the per student voucher.

[17] Municipal schools cannot charge fees, but parents also declare payments to those schools, through parent associations, for example. We also focus on municipal schools where more than 85% of the students pay less than Ch$5.000 monthly. We find that this group of schools represents 72% of the population of public students, and that the amount of money received from parents represents approximately 5% of their funds (excluding municipal transfers).

Figure 2: Distribution of language test scores in PS schools by fees charged



Results

Table 1 shows the treatment parameters estimated considering all geographical areas, and all the schools where more than 85% of the students declare that they pay less than Ch$5.000 (i.e. low levels of additional funding). We find a positive and statistically significant *ATE* and *TT*[18]. When we separate income groups, we find that even in low income groups the treatment effects are positive and large in magnitude (more than 25% of a standard deviation). Regression results are shown in Appendix B, where we obtain the usual results in the education literature: more per-capita income, parental education and pre-school education increases test scores, and girls attain better results than boys. In the selection equation we find that a higher price per unit of test score

[18] When we estimate using instrumental variables, we find a treatment parameter of 20.4 in language and 18 in math.

charged by private subsidized schools in a geographical area increases the probability of choosing a public school.

Table 1: Treatment Effects by income group, only schools that charge low fees. Without controlling for peer characteristicas

Language	Mean test score	Standard Deviation (SD) of test score	ATE*	ATE/SD	TT*	TT/SD
All	243.2	52.0	17.2	33%	19.1	37%
			(8.5)		(6.6)	
Income <Ch$100M	229.6	50.5	15.7	31%	17.5	35%
Income: Ch$100M-200M	244.9	50.7	18.3	36%	20.1	40%
Income: Ch$200M-300M	256.9	49.6	16.5	33%	18.1	36%
Income: Ch$300M-400M	265.1	49.1	17.9	37%	19.5	40%
Income: Ch$400M-600M	270.4	49.0	20.1	41%	21.5	44%
Income: Ch$600M-800M	276.4	49.4	23.9	48%	25.3	51%
Income: Ch$800M-1.200M	259.7	54.5	18.0	33%	19.7	36%
Income>Ch$1.200M	251.8	61.8	27.5	44%	29.3	47%
Math	Mean test score	Standard Deviation (SD) of test score	ATE*	ATE/SD	TT*	TT/SD
All	239.9	51.8	21.4	41%	17.1	33%
			(8.1)		(7.7)	
Income <Ch$100M	226.6	50.5	21.4	42%	16.8	33%
Income: Ch$100M-200M	241.9	50.4	21.7	43%	17.4	35%
Income: Ch$200M-300M	253.7	50.0	20.0	40%	15.9	32%
Income: Ch$300M-400M	260.8	48.4	21.3	44%	17.4	36%
Income: Ch$400M-600M	265.6	49.9	21.1	42%	17.4	35%
Income: Ch$600M-800M	271.8	50.9	22.6	44%	18.6	36%
Income: Ch$800M-1.200M	254.7	53.9	21.7	40%	18.1	34%
Income>Ch$1.200M	245.0	61.8	37.2	60%	34.8	56%

*Standard Errors (in parentheses) obtained using parametric bootstrapping

If we use different specifications, we find similar results, implying the results are robust. For example, when we run regressions including all schools (independent of the fee charged) and we control for school fees in the regressions, we find a similar *TT* [19], when we consider the same group of

[19] 18.2 in language and 23.23 in math.

students and schools. When we run regressions with all schools but without controlling for school fees we find a larger *TT* [20], as expected.

IV. ASSESSING THE IMPORTANCE OF PEER EFFECTS

The results described in the preceding section tell us that when we consider only schools that charge low additional fees (and thus similar total fees), voucher students achieve higher test scores when they attend a private voucher school instead of a public one. Thus theoretically, if we were to move a student from a private voucher school to a public school[21] our findings suggest that his test score would fall. If the sorting process and associated peer effects are important, this finding may be the result of being exposed to a better peer group in private subsidized schools, and not due to superior teaching by such schools. In this section we test this hypothesis.

Test formulation

Consider two polar scenarios:

- Case 1 *("Sorting is all that matters")*: Public and Private voucher schools are equally effective providers of education, and peer group quality is an important determinant of the student achievement. Thus, private voucher schools obtain better results only due to the sorting process and the associated

[20] 26.8 in language and 31.3 in math.

[21] In what follows we will consider two thought-experiments in which we move the student with and without his peers, and two scenarios (or cases), each of which describes a different reality regarding relative effectiveness of PS and public schools.

peer effect. Case 1 is consistent with Epple and Romano (1998), who assume that achievement (a) is an increasing function on the student's ability (x) and the mean ability of the student body in the school attended ($\bar{x}$) [22], i.e., $a = a(x, \bar{x})$.

- Case 2 *("School type is all that matters")*: Private voucher schools are more effective in the production of education, and peer abilities do not affect student achievement. Then, if d denotes school type, $a = a(x, d)$ using the previous notation. Thus, private voucher schools obtain better results simply because they *are better*. This case is consistent with the hypothesis that, since both types of schools face different incentives and restrictions, they exploit the student's characteristics differently, making potential test scores in each type of school different.

If Case 1 were true, a positive *TT* would be the result of a sorting process. Therefore, if we perform the experiment of moving a *single* voucher student to a public school (what we shall call Experiment 1), there should be a drop in his/her test scores. However, since voucher students obtain better results in private voucher schools only because of the composition of their class, if we perform the experiment of moving a voucher student *with all his classmates* to a public school (what we call Experiment 2), there should be no effect on test

[22] Sapelli (2003) discusses the assumption that only the mean of peer ability is important. He argues that other moments of the distribution of peer ability should affect the student's achievement. He finds that a larger standard deviation (SD) of ability in schools implies lower test scores, ceteris paribus. The issue is important, since we cannot change the mean by sorting, but we can lower the average of all schools' SD by sorting (i.e. average class SD can be much lower than population SD).

scores[23]. Alternatively, if Case 2 were true, the test scores should be reduced in both Experiment 1 *and* Experiment 2.

This is the intuition behind the test we perform in this section: we change our estimation strategy in order to compare the treatment parameters that result from Experiments 1 and 2. The results show whether Case 1 or Case 2 better describes the relative effectiveness of PS and public schools.

Estimation strategy

In this section we control for peer group characteristics in the output equation and estimate new treatment parameters using the coefficients we obtain. To this end, we calculate the mean and the standard deviation of the years of education of the mothers of all students in each class and include them as control variables in the outcome equation[24].

The new outcome equation is:

$$Y_D = X\beta^*_D + P\gamma^*_D + u^*_D$$

Where P is the vector of peer characteristics. Therefore, in the new outcome equation we control for the same individual-level characteristics as before (including educational achievement of both parents) and also for two class-level variables which are intended to capture peer effects. The inclusion of those new variables results in possibly different coefficients (denoted by β^*_D).

[23] Note that both experiments still constitute a partial equilibrium analysis.
[24] Notice that in the previous section we controlled for the education of the student's mother, and now we are controlling for the average education of the mothers of all the students in the class. Hence, in the first section we controlled for the student's characteristics, but now we are also controlling for peer group characteristics.

When we control for peer group characteristics in the outcome regression, we may be violating the no-feedback condition for treatment effect estimation (see Heckman (2001)), because it is possible that since more educated parents choose better schools, we will find better peers attending better schools. Thus, peer group characteristics may be related to school type. For this reason, our preferred treatment effect estimate is the one obtained in the previous section.

It is important to note that in this section we are not estimating peer effects, but controlling for peer characteristics (and the same is true for all the socioeconomic variables that we control for, since we are not estimating an education production function). Furthermore, as peer composition is endogenous, the coefficients for those variables cannot be viewed as estimates of structural parameters. Much work remains to be done on estimating peer effects. For instance, it is not clear how peer effects operate (are they symmetrical?) and how they can be identified (due to the causality problem, since peer characteristics may be endogenous).

Using the new coefficients obtained when we control for peer group characteristics, we estimate two treatment parameters:

- TT_1 is the treatment parameter that results if we perform Experiment 1, that is, if we move a *single* student to a public school (the expected test score change on moving the student is the negative of TT_1). Since in this experiment the student leaves his peers behind, after changing schools his new peers are municipal school students. For this reason, when we estimate TT_1 we use the average peer group characteristics in municipal schools ($\overline{P_{MUN}}$) to predict the student's score at the public school. To predict the student's results at his initial (PS) school, we use his actual peer group characteristics.

- TT_2 is the treatment parameter that results if we perform Experiment 2, that is, if we move the student *along with all his classmates* to a public school (the expected test score change on moving the student and his classmates is the negative of TT_2). As this experiment moves the student with all his peers, we use the student's initial peer group characteristics (that is, the characteristics of the peer group he had at the PS school) to predict test scores in the municipal school. Then, if Case 1 were true and all the difference between public and PS schools were due to peer effects, TT_2 should be zero.

If we call the treatment parameter estimated in the previous section TT_0, then we define TT_0^* as TT_0 using the new regression coefficients estimated when we include peer group characteristics (P) as additional control variables (we call these new estimated parameters $\beta^*_D, \rho^*_D, \sigma^*_D, \theta^*$ and γ^*_D). We can relate the treatment parameters resulting from both experiments with TT_0* as follows:

$$\begin{aligned} TT_1 &= X\left(\beta_1^* - \beta_0^*\right) + \left[P\gamma_1^* - \overline{P_{MUN}}\gamma_0^*\right] + \left(\rho_1^*\sigma_1^* - \rho_0^*\sigma_0^*\right)\lambda\left(-Z\theta^*\right) \\ &= TT_0^* + \left[P\gamma_1^* - \overline{P_{MUN}}\gamma_0^*\right] \end{aligned}$$

$$\begin{aligned} TT_2 &= X\left(\beta_1^* - \beta_0^*\right) + \left[P\left(\gamma_1^* - \gamma_0^*\right)\right] + \left(\rho_1^*\sigma_1^* - \rho_0^*\sigma_0^*\right)\lambda\left(-Z\theta^*\right) \\ &= TT_0^* + \left[P\left(\gamma_1^* - \gamma_0^*\right)\right] \end{aligned}$$

TT_1 can be separated into two components: the first is the difference in potential test scores between private subsidized and municipal schools, and the second is the difference in peer group composition between those schools. However, TT_2 includes only the first component, since in this case we maintain the same peer group when we predict outcomes at the two types of schools.

In the formula for TT_2, where we maintain the same peer composition, if TT_0^* were zero and $\gamma_0^* = \gamma_1^*$, then TT_2 would be zero, implying that potential test scores would be equal at both types of schools. This is what occurs in Case 1 described above, where sorting is what produces differences between school results and schools themselves are similarly effective.

If TT_0^* were positive and $P\left(\gamma_1^* - \gamma_0^*\right) \geq 0$ [25], a positive TT_2 results. That is, potential test scores are higher in PS schools even for the same peer composition. That is what is implied in Case 2, where sorting is irrelevant and superior PS performance is due to better teaching at PS schools.

Since the peer group in PS schools is better (in the sense that their mothers' education is higher), we should expect $TT_1 > TT_2$. The difference should be larger if we work with all schools as opposed to only working with schools that charge low fees. This is because in the complete sample the average mother's education is 10.9 years in private subsidized schools, and 8.8 years in municipal schools. But when we focus on schools that charge low fees, this difference is reduced (because the average mother's educational attainment

[25] And also if $P\left(\gamma_1^* - \gamma_0^*\right) < 0$, but smaller in magnitude than TT_0^*.

in private subsidized schools is 9.7 years). Thus, for this group of schools sorting is not as important as it is for the population as a whole.

Results

Table 2 shows the treatment parameters obtained when we control for mean and standard deviation of the education of the mothers of all the students in the class. The most important result is that TT_2 is still large in magnitude and statistically significant. Thus, we reject the hypothesis that schools are equally effective (the hypothesis that Case 1 holds in the data) and find that even when controlling for peer characteristics, PS schools are more effective.

Table 2: Treatment Effects by income group, only schools that charge low fees. Controlling for peer characteristicas

Language	Mean test score	Standard Deviation (SD) of test score	TT_1*	TT_1*/SD	TT_2*	TT_2*/SD
All	243.2	52.0	30.8	59%	28.0	54%
			(5.5)		(5.5)	
Income <Ch$100M	229.6	50.5	25.9	51%	27.4	54%
Income: Ch$100M-200M	244.9	50.7	31.7	62%	28.6	56%
Income: Ch$200M-300M	256.9	49.6	32.9	66%	26.5	53%
Income: Ch$300M-400M	265.1	49.1	36.9	75%	28.6	58%
Income: Ch$400M-600M	270.4	49.0	40.2	82%	29.6	60%
Income: Ch$600M-800M	276.4	49.4	45.0	91%	33.5	68%
Income: Ch$800M-1.200M	259.7	54.5	35.1	64%	26.1	48%
Income>Ch$1.200M	251.8	61.8	45.0	73%	29.6	48%
Math	Mean test score	Standard Deviation (SD) of test score	TT_1*	TT_1*/SD	TT_2*	TT_2*/SD
All	239.9	51.8	31.1	60%	28.4	55%
			(5.9)		(5.9)	
Income <Ch$100M	226.6	50.5	27.6	55%	29.1	58%
Income: Ch$100M-200M	241.9	50.4	31.3	62%	28.3	56%
Income: Ch$200M-300M	253.7	50.0	32.8	66%	26.6	53%
Income: Ch$300M-400M	260.8	48.4	36.6	76%	28.7	59%
Income: Ch$400M-600M	265.6	49.9	37.8	76%	27.6	55%
Income: Ch$600M-800M	271.8	50.9	40.2	79%	29.1	57%
Income: Ch$800M-1.200M	254.7	53.9	35.5	66%	26.9	50%
Income>Ch$1.200M	245.0	61.8	52.7	85%	37.9	61%

*Standard Errors (in parentheses) obtained using parametric bootstrapping

As expected, we find that $TT_1 > TT_2$. Nonetheless, the difference between them is small in magnitude, because there are no big differences in peer group characteristics between the schools we are focusing on. What is surprising is that both TT_1 and TT_2 are bigger than either TT_0 or TT_0^*. This finding is the result of a larger coefficient on peer characteristics in private subsidized schools than in municipal schools. That is, TT_0 and TT_0^* are similar in magnitude, but since $\gamma_1^* > \gamma_0^*$, TT_1 and TT_2 are larger than TT_0. In short, some of the treatment effect associated with PS schools that we estimate in this section is the product of such schools making better use of peer group characteristics than municipal schools.

Summarizing, average peer characteristics in public schools and in the PS schools are not very different (i.e. there is not much sorting), and our results show that PS schools make better use of the characteristics of all students. Regression results are shown in Appendix C.

V. FINAL REMARKS

This paper considers an issue that is a central to any evaluation of a voucher system: the relative performance of private and public voucher schools. To examine this question, it is vital to compare schools with similar budgets, which is why we focus on schools that charge low fees to estimate treatment parameters.

In the first section of the paper we estimate treatment parameters (average treatment effect and the effect of treatment on the treated), where treatment is defined as attending a private voucher school instead of a public one. We find positive treatment parameters that are large in magnitude and statistically significant (more than one third of a standard deviation in test scores).

In the second section of the paper we evaluate the importance of peer effects on the treatment parameters estimated earlier. We estimate a new treatment effect, the result of a thought-experiment in which we move a student with all his/her classmates from a private to a public voucher school. After controlling for socioeconomic characteristics of the students and now also of their peers, we still find a treatment parameter that is positive, large in magnitude and statistically significant (around 50% of a standard deviation on test scores). Therefore we conclude that the positive treatment effects are *not* solely due to the peer effect and sorting. In short, the peer effect is not important enough to invalidate results that do not take them into consideration, or to change the way that such results are interpreted.

APPENDIX A

Description of the variables used in the estimation
(178.122 observations)

Variable	Mean	Std. Dev.
Language test score	251.88	52.32
Math test score	247.97	52.19
Dummy PS	0.47	0.50
Dummy income <Ch$100M	0.28	0.45
Dummy income: Ch$100M-200M	0.38	0.49
Dummy income: Ch$200M-300M	0.15	0.35
Dummy income: Ch$300M-400M	0.07	0.25
Dummy income: Ch$400M-600M	0.06	0.24
Dummy income: Ch$600M-800M	0.02	0.14
Dummy income: Ch$800M-1.200M	0.02	0.13
Dummy income>Ch$1.200M	0.01	0.09
Incomplete elementary education (mother)	0.23	0.42
Complete elementary education (mother)	0.09	0.29
Incomplete secondary education (mother)	0.22	0.41
Complete secondary education (mother)	0.13	0.34
Technical educatin (mother)	0.16	0.36
Technical tertiary education (mother)	0.11	0.31
Universitary education (mother)	0.05	0.23
Incomplete elementary education (father)	0.22	0.42
Complete elementary education (father)	0.10	0.29
Incomplete secondary education (father)	0.21	0.41
Complete secondary education (father)	0.13	0.33
Technical educatin (father)	0.17	0.37
Technical tertiary education (father)	0.09	0.29
Universitary education (father)	0.08	0.27
Dummy gender (1: male)	0.50	0.50
Family Size	5.17	2.58
Dummy failed a class (1: no)	0.90	0.30
Age	9.57	1.81
Dummy Preschool education (1: yes)	0.87	0.33
Price per unit of test score, PS schools	34.45	21.16
Price per unit of test score, MUN schools	1.49	2.44

APPENDIX B

Estimation Results

Outcome Equation: Language test scores

	Private subsidized schools		Municipal schools	
	Number of obs.= 86800			
	Wald chi2(24)=1071.85		Wald chi2(24)=3886.45	
	Log likelihood = -140707.9		Log likelihood = -378269.7	
	Prob > chi2 = 0.0000		Prob > chi2 = 0.0000	
	Coefficient	Standard Error	Coefficient	Standard Error
Dummy income: Ch$100M-200M	7.48	0.90	6.06	0.58
Dummy income: Ch$200M-300M	10.65	1.57	11.43	1.01
Dummy income: Ch$300M-400M	15.68	2.35	14.90	1.21
Dummy income: Ch$400M-600M	18.29	2.53	15.06	1.37
Dummy income: Ch$600M-800M	24.97	3.29	17.33	1.91
Dummy income: Ch$800M-1.200M	14.54	3.70	12.10	2.42
Dummy income>Ch$1.200M	18.63	8.08	6.55	3.82
Complete elementary education (mother)	4.24	1.37	4.12	0.83
Incomplete secondary education (mother)	8.20	1.51	6.09	0.68
Complete secondary education (mother)	17.34	1.73	15.01	0.86
Technical educatin (mother)	14.46	1.77	14.31	0.86
Technical tertiary education (mother)	18.14	2.31	17.92	1.05
Universitary education (mother)	23.75	2.43	25.58	1.62
Complete elementary education (father)	3.43	1.31	2.21	0.81
Incomplete secondary education (father)	7.54	0.98	5.51	0.63
Complete secondary education (father)	12.93	1.67	11.69	0.77
Technical educatin (father)	14.31	1.57	10.56	0.79
Technical tertiary education (father)	15.88	1.80	14.89	1.14
Universitary education (father)	22.97	2.51	20.97	1.37
Dummy gender (1: male)	-7.66	0.84	-6.72	0.55
Family Size	-0.97	0.20	-1.01	0.15
Dummy failed a class (1: no)	28.27	1.29	25.50	0.69
Age	0.23	0.25	0.41	0.11
Dummy Preschool education (1: yes)	3.80	1.25	0.98	0.68
constant	211.50	8.77	200.16	2.22

(standard errors adjusted for clustering on comuna)

Outcome Equation: Math test scores

	Number of obs.= 86822			
	Wald chi2(24)=989.02		Wald chi2(24)=3549.52	
	Log likelihood = -140817.3		Log likelihood = -379180.9	
	Prob > chi2 = 0.0000		Prob > chi2 = 0.0000	

	Private subsidized schools		Municipal schools	
	Coefficient	Standard Error	Coefficient	Standard Error
Dummy income: Ch$100M-200M	6.19	1.00	6.76	0.57
Dummy income: Ch$200M-300M	9.62	1.41	12.21	0.99
Dummy income: Ch$300M-400M	13.64	2.37	15.08	1.22
Dummy income: Ch$400M-600M	13.59	2.89	15.41	1.62
Dummy income: Ch$600M-800M	17.98	3.78	18.58	2.32
Dummy income: Ch$800M-1.200M	11.14	4.31	11.94	2.33
Dummy income>Ch$1.200M	16.29	7.07	1.29	3.68
Complete elementary education (mother)	4.97	1.42	5.04	0.78
Incomplete secondary education (mother)	7.28	1.28	6.77	0.61
Complete secondary education (mother)	17.65	1.60	15.13	0.79
Technical educatin (mother)	14.48	1.50	15.09	0.89
Technical tertiary education (mother)	17.45	2.17	18.10	1.07
Universitary education (mother)	24.94	2.77	25.91	1.48
Complete elementary education (father)	3.90	1.41	2.42	0.81
Incomplete secondary education (father)	6.97	1.15	5.24	0.59
Complete secondary education (father)	12.30	1.70	11.04	0.66
Technical educatin (father)	13.70	1.67	10.89	0.81
Technical tertiary education (father)	13.74	1.54	13.74	1.07
Universitary education (father)	24.05	2.37	19.03	1.32
Dummy gender (1: male)	4.53	0.93	4.94	0.56
Family Size	-0.85	0.19	-0.80	0.10
Dummy failed a class (1: no)	26.70	1.43	24.99	0.73
Age	0.24	0.30	0.37	0.11
Dummy Preschool education (1: yes)	4.23	1.25	1.64	0.67
constant	208.44	8.68	190.07	2.21

(standard errors adjusted for clustering on comuna)

Selection Equation: PS school

	Coefficient	Standard Error
Dummy income: Ch$100M-200M	0.08	0.02
Dummy income: Ch$200M-300M	0.16	0.04
Dummy income: Ch$300M-400M	0.17	0.06
Dummy income: Ch$400M-600M	0.24	0.07
Dummy income: Ch$600M-800M	0.21	0.08
Dummy income: Ch$800M-1.200M	0.25	0.06
Dummy income>Ch$1.200M	0.33	0.21
Complete elementary education (mother)	0.00	0.02
Incomplete secondary education (mother)	0.06	0.02
Complete secondary education (mother)	0.11	0.03
Technical educatin (mother)	0.18	0.03
Technical tertiary education (mother)	0.19	0.04
Universitary education (mother)	0.17	0.05
Complete elementary education (father)	0.00	0.02
Incomplete secondary education (father)	0.03	0.02
Complete secondary education (father)	0.05	0.03
Technical educatin (father)	0.08	0.03
Technical tertiary education (father)	0.05	0.04
Universitary education (father)	0.04	0.05
Dummy gender (1: male)	-0.04	0.02
Family Size	0.00	0.00
Dummy failed a class (1: no)	0.04	0.03
Age	0.00	0.00
Dummy Preschool education (1: yes)	-0.05	0.02
Price per unit of test score, PS schools	-0.01	0.00
Price per unit of test score, MUN schools	0.02	0.02
constant	-0.58	0.12

Language:	Coefficient	Standard Error
rho PS	-0.18	0.11
sigma PS	48.32	0.79
lambda PS	-8.62	5.31
rho MUN	0.20	0.08
sigma MUN	48.62	0.45
lambda MUN	9.88	4.05

Math:	Coefficient	Standard Error
rho PS	-0.26	0.10
sigma PS	48.90	1.00
lambda PS	-12.69	5.08
rho MUN	0.19	0.09
sigma MUN	48.78	0.45
lambda MUN	9.37	4.29

(standard errors adjusted for clustering on comuna)

APPENDIX C

Estimation Results: controlling for peer group characteristics

Outcome Equation: Language test scores

	Private subsidized schools		Municipal schools	
	Number of obs.= 86799			
	Wald chi2(24)=1564.00		Wald chi2(24)=3981.11	
	Log likelihood = -140336.9		Log likelihood = -377621.5	
	Prob > chi2 = 0.0000		Prob > chi2 = 0.0000	
	Coefficient	Standard Error	Coefficient	Standard Error
Dummy income: Ch$100M-200M	4.60	0.76	4.25	0.56
Dummy income: Ch$200M-300M	5.56	1.32	7.41	0.88
Dummy income: Ch$300M-400M	9.90	1.93	9.38	1.04
Dummy income: Ch$400M-600M	10.56	2.27	8.62	1.38
Dummy income: Ch$600M-800M	16.53	2.72	10.23	1.90
Dummy income: Ch$800M-1.200M	7.18	3.46	7.81	2.18
Dummy income>Ch$1.200M	4.38	5.46	1.91	3.73
Complete elementary education (mother)	2.19	1.26	3.50	0.83
Incomplete secondary education (mother)	3.87	1.27	3.27	0.65
Complete secondary education (mother)	11.27	1.50	11.06	0.76
Technical educatin (mother)	8.62	1.47	9.90	0.78
Technical tertiary education (mother)	10.31	2.30	11.86	0.95
Universitary education (mother)	16.16	2.30	19.09	1.49
Complete elementary education (father)	2.57	1.25	1.92	0.81
Incomplete secondary education (father)	4.79	0.96	3.49	0.62
Complete secondary education (father)	9.40	1.46	9.14	0.74
Technical educatin (father)	10.62	1.44	7.68	0.76
Technical tertiary education (father)	10.89	1.87	10.93	1.06
Universitary education (father)	16.11	2.06	16.42	1.26
Dummy gender (1: male)	-6.56	0.83	-6.25	0.55
Family Size	-0.76	0.17	-0.93	0.14
Dummy failed a class (1: no)	24.35	1.52	24.46	0.71
Age	0.23	0.25	0.42	0.11
Dummy Preschool education (1: yes)	2.37	1.10	0.61	0.71
Average years of education of the mother at the c	6.84	0.72	5.40	0.41
SD of years of education of the mother at the cla	0.23	1.83	-0.26	1.25
constant	156.65	14.56	155.54	6.59

(standard errors adjusted for clustering on comuna)

Outcome Equation: Math test scores

	Private subsidized schools		Municipal schools	
	Number of obs.= 86821			
	Wald chi2(24)=1380.98		Wald chi2(24)=3402.21	
	Log likelihood = -140512		Log likelihood = -378585.7	
	Prob > chi2 = 0.0000		Prob > chi2 = 0.0000	
	Coefficient	Standard Error	Coefficient	Standard Error
Dummy income: Ch$100M-200M	3.61	0.90	5.00	0.57
Dummy income: Ch$200M-300M	4.98	1.27	8.27	0.90
Dummy income: Ch$300M-400M	8.42	2.07	9.74	1.16
Dummy income: Ch$400M-600M	6.58	2.60	9.17	1.59
Dummy income: Ch$600M-800M	10.37	3.35	11.65	2.15
Dummy income: Ch$800M-1.200M	4.52	3.55	7.73	2.26
Dummy income>Ch$1.200M	3.34	5.01	-3.39	3.57
Complete elementary education (mother)	3.08	1.40	4.45	0.79
Incomplete secondary education (mother)	3.33	1.20	4.04	0.60
Complete secondary education (mother)	12.13	1.51	11.25	0.75
Technical educatin (mother)	9.16	1.34	10.75	0.77
Technical tertiary education (mother)	10.35	2.24	12.12	0.98
Universitary education (mother)	18.09	2.76	19.52	1.39
Complete elementary education (father)	3.15	1.37	2.12	0.81
Incomplete secondary education (father)	4.49	1.10	3.29	0.58
Complete secondary education (father)	9.13	1.52	8.56	0.66
Technical educatin (father)	10.39	1.61	8.09	0.80
Technical tertiary education (father)	9.22	1.52	9.92	0.96
Universitary education (father)	17.82	1.94	14.62	1.29
Dummy gender (1: male)	5.54	0.95	5.43	0.56
Family Size	-0.67	0.17	-0.72	0.10
Dummy failed a class (1: no)	23.19	1.56	23.97	0.75
Age	0.25	0.30	0.39	0.11
Dummy Preschool education (1: yes)	2.93	1.13	1.31	0.69
Average years of education of the mother at the	6.18	0.65	5.21	0.41
SD of years of education of the mother at the cla	0.06	1.54	-0.26	1.39
constant	159.35	11.81	146.40	6.81

(standard errors adjusted for clustering on comuna)

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